viscera; while the widespread contraction of the muscle system has probably also a good effect. How pronounced are the dynamic effects induced by completely abandoning oneself to a fit of crying is shown by the exhaustion which it entails. It is partly through this exhaustion that crying induces sleep; we hear of "crying oneself to sleep," though this must be but a very crude explanation of the phenomenon. The tendency of women to cry should, of course, be kept within proper bounds, but certainly harm may result from its complete suppression, as Tennyson recognises in the line—

> "She must weep or she will die."

It is said that women who are able to find relief in tears keep their youth longer than those who repress them. The internal cankering action "like a worm i' the bud" of pent-up emotion is not only a beautiful poetic conceit, but a profound physiological truth. In short, strong emotion should receive expression—"give sorrow words."

SIGHING.

"A sigh is a deep thoracic respiration, with retraction of the abdomen."[1] The retraction of the abdominal muscles leads to a compression of the splanchnic veins. This compression is probably increased by slight descent of the diaphragm. The blood is thus pressed out of those veins into the right heart, and the flow into this chamber is further favoured by the deep inspiration which also aids the circulation through the lungs. A more common cause of sighing I believe to be shallow breathing, however induced. Thus sadness and a sense of weariness or boredom are wont to be attended by shallow breathing, and in all of them sighing is frequent. In consequence of this shallow breathing, blood-aeration lags behind, and the blood tends to accumulate in the right heart and systematic veins. The sigh benefits by promoting the aeration of the blood and quickening the pulmonary circulation, and it is for similar reasons that sighing is apt to occur during a state of "breathless attention"—when the attention, *i.e.*, is so strained that one forgets, as it were, to breathe adequately.

YAWNING.

There can be little doubt that one of the objects of yawning is the exercise of muscles which have been for a long time quiescent, and the acceleration of the blood and lymph flow which has, in consequence of this quiescence, become sluggish. Hence its frequency after one has remained for some time in the same position—*e.g.* when waking in the morning. Co-operating with this cause is sleepiness and the shallow breathing which it entails. This factor, as well as muscle-quiescence, is apt to attend the sense of boredom which one experiences in listening to a dull sermon. Hence it is that the bored individual is apt to yawn. As in the case of sighing, the deep breath which accompanies the act of yawning compensates for the shallow breathing, which is so apt to excite it.

ON THE ASCENT OF WATER IN TREES.[2]

WITHIN the last few years the problem of the ascent of water has entered on a new stage of existence. The researches which have led to this new development are of such weight and extent that they might alone occupy our time. It will be necessary therefore to avoid, as far as possible, going into ancient history. But it will conduce to clearness to recall some of the main stepping-stones in the progress of the subject.

The two questions to be considered are: (1) What is the path of the ascending water? (2) What are the forces which produce the rise?

(1) The first question has gone through curious vicissitudes. The majority of earlier writers assumed that the water travelled in the vessels. This was not, however, a uniform view. Cæsalpinus, 1583, seems (Sachs' "History of Botany," English Trans., p. 451) to have thought that water moved by imbibition in the "nerves." Malpighi and Ray held that the vessels serve for air, and the wood fibres for the ascent of water. Hales ("Vegetable Staticks," p. 130), who believed in the "sap-vessels" as conduits, speculated on the passage upwards of water between the wood and the bark. Also (*loc. cit.* p. 19), that water may travel as vapour not in the liquid state. In the present century Treviranus (Sachs' "History"), 1835, held that water travelled in vessels; De Candolle, 1832, that the intercellular spaces were the conduits. In Balfour's "Manual of Botany," 1863, vessels, cells, and intercellular spaces are spoken of as transmitting the ascending water.

The change in botanical opinion was introduced by the great authority of Sachs,[1] who took up Unger's view[2] that the transpiration current travels in the thickness of the walls as water of imbibition.

Then followed the reaction against the imbibitionists—a reaction which has maintained its position up to the present time. Boehm, who had never adopted the imbibition theory, must have the credit of initiating this change; his style was confused and his argument marred by many faults, but the reaction should in fairness be considered as a conversion to his views, as far as the path of the travelling water is concerned. Nevertheless, it was the work of others who principally forced the change on botanists—*e.g.* von Höhnel (*Pringsheim's Jahrb.* xii., 1879), Elfving (*Bot. Zeitung*, 1882), Russow (*Bot. Centr.* xiii., 1883), R. Hartig ("Ueber die Vertheilung," &c., *Untersuchungen aus dem Forst. Bot. Inst. zu München*, ii. and iii.), Vesque (*Ann. Sc. Nat.* xv. p. 5, 1883), Godlewski (*Pringsheim's Jahrb.* xv., 1884), and others.

(2) The second question has a curious history, and one that is not particularly creditable to botanists generally. It has been characterised by loose reasoning, vagueness as to physical laws, and a general tendency to avoid the problem, and to scramble round it in a mist of *vis à tergo*, *capillarity*, *Jamin chains*, *osmosis*, and *barometric pressure*.

An exception to this accusation (to which I personally plead guilty) is to be found in Sachs' imbibition theory, in which, at any rate, the barometric errors were avoided, though it has difficulties of its own, as Elfving has pointed out.

But the most hopeful change in botanical speculation began with those naturalists who, concluding that no purely physical causes could account for the facts, invoked the help of the living elements in the wood. To Westermaier (*Deutsch Bot. Ges.* Bd. i., 1883, p. 371) and Godlewski (*Pringsheim's Jahrb.* xv., 1884) is due the credit of this notable advance, for whether future research uphold or destroy their conclusions, it claims our sympathy as a serious facing of the problem by an ingenious and rational hypothesis.[3]

We may pass over the cloud which arose to witness for and against these theories, and proceed at once to Strasburger's great work (*Leitungsbahnen*, 1891), in which, with wonderful courage and with the industry of genius, he set himself to work out the problem *de novo*, both anatomically and physiologically. In my opinion it is difficult to praise too highly this great effort of Strasburger's.

Strasburger's general conclusion is now well known. He convinced himself that liquid can be raised to heights greater than that of the barometric column in cut stems, in which the living elements have been killed. Therefore, the cause of the rise could not be (1) barometric pressure, (2) nor root pressure, (3) nor could it be due to the action of the living elements of the wood. His conclusions may be stated as follows:—

(*a*) The ascent of water is not dependent on living elements, but is a purely physical phenomenon.

(*b*) None of the physical explanations hitherto made are sufficient to account for the facts.

Strasburger has been most unjustly depreciated, because his book ends in this confession of ignorance. I do not share such a view. I think to establish such distinct, though negative, conclusions would be, in this most nebulous of subjects, an advance of great value. Whether he has established these conclusions must of course be a matter of opinion. To discuss them both would be to go over 500 pages of Strasburger's book, and will not here be attempted. Conclusion (*a*) that the ascent is not dependent on living elements must, however briefly, be discussed, because it is here that the roads divide. If we agree with Strasburger, we know that we must seek along the physical line; if we differ from him, we are bound to seek for the missing evidence of the action of the living elements.

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[1] L. Hill: *Journal of Physiology*, vol. xv. p. 48.

[2] A paper read before Section K of the British Association at the Liverpool meeting, by Francis Darwin, F.R.S. (Revised January 20, 1897).

[1] *Physiol. Végétale* (French Trans.), 1868, p. 235, and more fully in the *Lehrbuch*. Sachs also partially entertained Quincke's well-known suggestion of movement of a film of water on the surface of vessels.

[2] *Sitz. k.k. Akad. Wien*, 1868. Dixon's and Joly's paper in the *Annals of Botany*, September 1895, gives evidence in favour of a certain amount of movement of the imbibed water.

[3] It is of interest to note that Hales, in speaking of the pressure which he found to exist in bleeding trees, says: "This force is not from the root only, but must also proceed from some power in the stem and branches." ("Veg. Staticks," 1727, p. 110.)

Schwendener's Criticism.—Perhaps the best plan will be to consider the most serious criticism that has been published of Strasburger's work, namely Schwendener's paper "Zur Kritik," &c. (*K. Preuss. Akad.* 1892, p. 911).

Schwendener objects that although a continuous column of water cannot be raised by air pressure to a greater height than that of the barometric column, yet when broken into a number of columns, as in the case of a Jamin chain, that a column considerably over 10 m., even as much as 13 or 14 m., of water can be suspended. This, though not fatal to Strasburger's conclusions, is no doubt a serious criticism. For if 13 m. can be supported, some of Strasburger's experiments are inconclusive. He finds that a branch can suck up a poisonous fluid to over 10 m., and, as above explained, argues that all ascent above that height, not being due to barometric pressure or to the living elements (since the wood is poisoned), is for the present inexplicable. But, if Schwendener is right, the effect above 10 m. may have been due to atmospheric pressure. Askenasy (*loc. cit. infra*, 1895, p. 6) objects to Schwendener that the supposed action cannot be continuous. By repeating the diminution of air pressure at the upper end, the movement of water becomes less and less, and sinks to almost nothing. Askenasy adds, moreover, that the amount of water which could be raised according to Schwendener's theory would be very small.

One difficulty about Schwendener's theory is that the result depends on the length of the elements of which the chain is made up (such element being a water column, *plus* an air bubble.) In his paper "Ueber das Saftsteigen" (*K. Preuss. Akad.* 1886, p. 561), he finds that the elements of the chain in *Fagus* equal in round numbers 0·5 mm. In his paper (*K. Preuss. Akad. Sitz.* 1893, p. 842), "Wasserbewegung in der Jamin'schen Kette," he finds the element in *Acer pseudo-platanus* = 0·9 mm., in *Acer platanoides* and *Ulmus effusa* = 0·2. But the calculation (1892, p. 934) is based on the existence of a chain in which the water columns are each 10 mm. in length, a condition of things which he allows does not occur in living trees.

But even if we allow Schwendener to prove theoretically the possibility of a Jamin chain being raised to a height much greater than that of a barometric column, I do not think he invalidates Strasburger's position. Schwendener's idea necessitates the travelling of a Jamin chain as a whole, *i.e.* the translation not only of water, but of air bubbles. But this cannot (as Strasburger points out) apply to his experiments on conifers, in which the movement of air to such an extent is impossible ("Ueber das Saftsteigen," *Hist. Beiträge*, v. 1893, p. 50). And for the case of dicotyledonous woods, Strasburger has shown that the movement of air is excluded by the fact that transverse walls occur in the vessels at comparatively short distances. In Aristolochia the sections may be as long as 3 m., but in ordinary woods, according to Adler (as quoted by Strasburger), we get: Alnus, 6 cm.; Corylus, 11 cm.; Betula, 12 cm.; Quercus, 57 cm.; Robinia, 69 cm. These facts seem impossible to reconcile with Schwendener's views.

Action of the Poisonous Fluids in Strasburger's Experiments.—The question whether the living elements are killed in Strasburger's experiments is of primary importance in the problem.

Schwendener does not criticise it at length; he seems to assume (*Zur Kritik, loc. cit.*, 1892, p. 935)—as far as I can understand—that since the death of the tissues extends gradually from the cut end upwards, there are living cells in the upper part which may still be effective. He also doubts "whether the cells were always killed at once." The first objection of Schwendener's may or may not be sound, but in any case it does not (as Strasburger points out) account for the experiment (*Hist. Beitr.* v. p. 12) in which an oak stem was poisoned by picric acid, and three days afterwards was placed in fuchsin-picric. The second reagent had to travel in tissues already killed with picric acid, yet a height of 22 m. was reached.

The question whether the reagents kill the cells in Strasburger's experiments does not lend itself to discussion. It is difficult to see how they should escape, and we have Strasburger's direct statement that the living tissues were visibly killed. It must not be forgotten that in some of his experiments the death of the tissues was produced by prolonged boiling, not by poisons (*Leitungsbahnen*, p. 646). Thus the lower 12 m. of a Wistaria stem were killed in this way, yet liquid was sucked up to a height of 108 cm. In the *Histolog. Beitr.* v. p. 64, he has repeated his air-pump experiment, using a boiled yew branch, and found that eosin was sucked up from a vessel in which almost complete vacuum was established, so the action of living elements and of atmospheric pressure was excluded.

On the whole, the balance of evidence is, in my judgment, against the belief that the living elements are necessary for the rise of water. In other words, I think we should be justified, from Strasburger's work, in seeking the cause of ascent in the action of purely physical laws.

Strasburger's general argument from the structure of wood.—It seems sometimes to be forgotten that, apart from the physiological or experimental evidence, there is another line of argument founded on the structure of wood. Strasburger's unrivalled knowledge allows him to use this argument with authority, and he seems to me to use it with effect. Thus (*Hist. Beitr.* v. p. 17) he points out that though in coniferous wood the action of the living elements in pumping water is conceivable, yet this is far from being universally the case. He points out that in the monocotyledons such theories meet with almost unconquerable difficulties. This is, he says, especially the case in Dracæna. He goes on to point to difficulties in the case of such dicotyledons as Albizzia. The case may perhaps best be put in the generalised manner that Strasburger himself employs (*loc. cit.* p. 20). If the living elements are of such importance as Godlewski, Westermaier, and Schwendener hold, we ought not to find these difficulties; we ought rather to find structural peculiarities pointing distinctly to the existence of such functions. For instance, we ought to find the tracheal water-path actually interrupted by living elements, which might act like a series of pumping stations one above the other. It should, however, be remembered that if we deny the importance of the medullary rays and other living elements in raising water, we ought to be able to point more clearly than we can at present to the function of the medullary rays and to structural adaptations to these functions.

The work of Dixon and Joly and of Askenasy.—I now pass on to the recent work in which Strasburger's indications to search along a purely physical line have been followed. In the paper of Dixon and Joly (*Proc. Roy. Soc.*, vol. lvii. 1894, No. 340), the suggestion was for the first time made that the raising of water to the tops of trees depends on the quality which water possesses of resisting tensile stress. To most botanists the existence of this quality is a new idea. To believe that columns of water should hang in the tracheals like solid bodies, and should, like them, transmit downwards the pull exerted on them at their upper ends by the transpiring leaves, is to some of us equivalent to believing in ropes of sand. The idea is more fully treated in the *Phil. Trans.* vol. clxxxvi., and in the *Annals of Botany*, vol. viii. The same leading idea occurred independently to Askenasy, who has published it in the *Verhand. a. d. naturhist. med. Vereins Heidelberg*, N. F., Bd. v., 1895; and N. F., Bd. v., 1896.

Askenasy has earned the gratitude of his botanical readers by giving some of the evidence which demonstrates the existence of this property of water.[1] A tube a metre in length was filled by Donny with water, and the remaining space was as far as possible freed from air. When the tube was placed vertically, the water-column at the upper end hung there, and could not be made to break or free itself from the glass by violent shaking. Berthelot filled a thick-wall capillary tube completely with water at 28°–30° C.; it was allowed to cool to 18°, so that the space left by the shrinking of water was filled with air. It was then sealed up and again warmed to 28°–30°, so that the air was dissolved in the water. When it was allowed to cool again it retained its volume, filling the tube completely. A slight shake, however, allowed the water to break and return to its proper volume at 18° with the appearance of a bubble of air. In this experiment the water contained air, yet it seems to have been until recently assumed by some physicists that to show cohesion, water must be air-free. If this were the case, the application of the principle to plants would be impossible. Dixon and Joly have, however, proved that this is not so, and this forms an important part of their contribution to the subject.

They also[2] investigated the amount of tension which water under these circumstances will bear, and found it about equal to seven atmospheres. If, therefore, the leaves at the top of a tall tree can exert the requisite upward pull on the water in the trunk, it seems certain (if no other condition in the problem interfere) that the pull can be transmitted to the level of the ground. This opens up the question whether the leaves can exert this traction on the water in the tracheals, and what is equally important, Are there any factors in the problem incompatible with the theory?

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[1] He gives reference to Donny, *Poggendorff's Annalen*, 67 Bd. (143. Bd. d. g. R.), 1846, p. 562; Berthelot, *Annales de Chimie et de Physique*, S. 3, t. 30, 1850, p. 232; Worthington, *Proc. Roy. Soc.* vol. l., 1892, p. 423.

[2] *Phil. Trans.* vol. 186, p. 570. With ethyl alcohol Worthington records a tension of 17 atmospheres. See *Proc. R. Soc.*, vol. l.

(1) *The sucking force of the leaves.*—In Dixon and Joly's first paper (*Phil. Trans.* pp. 563, 567) they assume that tractional force is given by the meniscuses "formed in the membranous *réseau* of the evaporating cell-walls," as well as possibly by the osmotic action of the cells of the mesophyll. We shall take these theories in order. Our knowledge of the cell wall does not allow us to believe in the existence of pores visible with even the highest powers of the microscope. Dixon's more general expression (*Proc. Roy. Irish Acad.* Jan. 13, 1896, p. 767) "surface tension forces developed in the substance of the walls of the evaporating cells," is therefore preferable. But Askenasy seems to me to state the matter much more conveniently by using the term "imbibition" (*loc. cit.* 1895, p. 10). The force with which vegetable membranes, *e.g.* the thallus of Laminaria, absorb water, has been demonstrated by Reinke and others, and the existence of such a force is familiar to botanists.

Both Askenasy (*loc. cit.*) and Dixon and Joly (*Annals of Bot.*, September 1895) have pointed out that the force of imbibition, or the surface tension forces, as the case may be, can exert a tractional effect on the water in the tracheals, when the turgescence of the mesophyll has been destroyed. But Askenasy in his original paper (1895), Dixon in the January 1896 paper, and again Askenasy in his second paper (March 1896) have also considered the imbibitional or surface tension forces in connection with the turgescent cell. It must clearly be understood that this does not remove imbibition from the problem. The sun's heat causes the evaporation of the water with which the walls of the mesophyll cells are imbibed; this water is replaced by imbibition from the cell-sap. The concentration of the cell-sap so produced maintains the osmotic force of the cell, which again exerts suction on the water on the tracheals.[1]

I have now given, in its simplest form, the modern theory of the rise of water. Apart from the main idea, it combines the points of several familiar views. Imbibition becomes a factor of paramount importance, though not in the way that Sachs employs it. The suspended threads of water remind us of Elfving's capillary theory, while the living element factor is represented by the turgescent mesophyll cells.

Resistance.—It is not possible to discuss the question whether the tractional forces in the leaf are sufficient for the work imposed on them until we know what is the resistance to the passage of water through wood. For it is clear that the work done by the leaf includes not only the lifting of a given column, but the overcoming of the resistance to its flow.

The resistance to the flow of the transpiration current is in want of further investigation. Janse (*Pringsheim's Jahrb.* xviii., 1887, p. 1) has discussed the question, and points out (*loc. cit.* p. 36) that two kinds of resistance must be reckoned with. The first (which he calls statical) is illustrated by means of a cylinder of Pinus wood fixed to the short arm of a J tube filled with water, when it was found that in five days the level of water in the long arm was only one mm. above that in the short arm.[2] That is to say, when time enough is given, the resistance is practically nothing. Janse has also investigated the resistance to the passage of water flowing through wood at the rate of an ordinary transpiration current. His method seems to me open to criticism, but this is not the place to give my reasons. His experiments give a wide range of results. With *Pinus strobus* a pressure of water equal to ten times the length of the wood was required to force water through at a pace equal to the transpiration current. In Ginkgo the pressure was twenty-one times the length of the wood. Strasburger (*Leitungsbahnen*, p. 779) has repeated Janse's experiment, and finds a column "several times the length of the object" necessary. Nägeli ("Das Mikroskop," 2nd edit. p. 385) found that 760 mm. of mercury were needed to force water through fresh coniferous wood at the rate of $\frac{1}{21}$ mm. per second, *i.e.* at 180 mm. per hour. If we allow one metre per hour as a fair transpiration rate (Sachs' "Arbeiten," ii. p. 182), we get a pressure of 5 atmospheres required to produce such a flow. To return to Janse's experiments: even if we assume that the resistance (expressed in water) = 5 times the length, it is clear that with a tree 40 m. in height, the resistance of 20 atmospheres has to be overcome. This would not be a pressure greater than that which osmotic forces are able to exert, but when we come to a tree of 80 m. in height, and a resistance of 40 atmospheres, the thing becomes serious.[1] A great difficulty in the question of resistance is that the results hitherto obtained are (though here I speak doubtfully) much greater than those obtained by physicists for the resistance of water flowing in glass capillaries. Until this discrepancy is explained, it is rash to argue from our present basis of knowledge.[2]

Is the osmotic suck sufficient?—The osmotic force of a turgescent cell is usually measured by its power of producing hydrostatic pressure within the cell. Thus, De Vries ("Untersuchungen über d. mechanischen Ursachen der Zellstrecken," 1877, p. 118) investigated the force necessary to extend a plasmolysed shoot to its original length; Westermaier (*Deutsch. Bot. Ges.* 1883, p. 382) the weight necessary to crush a tissue of given area; Pfeffer (*Abh. k. Sächs. Ges.* 1893) the pressure exerted by growing roots; Krabbe (*K. Akad. Berlin* (*Abhandlungen*), pp. 57, 69, 1884) the pressure under which cambium is capable of maintaining its growth.

The figures obtained by these naturalists have a wide range; it may be said that the hydrostatic pressure varies between 3 and 20 atmospheres.

Another method is to ascertain the osmotic strength of the cell-sap in terms of a KNO_3 solution, and calculate the pressure which such a solution can produce. According to Pfeffer (Pfeffer, *Phys.* i. p. 53), 1 per cent. KNO_3 with artificial membrane gives a pressure of 176 cm. = 2·3 atmospheres. De Vries (*Pringsh. Jahrb.* xiv. p. 527) calculates that in a cell, a 0·1 equivalent solution (practically = 1 per cent.) gives a pressure of 3 atmospheres. We may therefore take it as between 2·5 and 3 atmospheres. Now, De Vries found that beetroot requires 6–7 per cent. KNO_3 to plasmolyse it; this would mean 15–21 atmospheres. I do not know what is the greatest pressure which has been estimated in this way. Probably Wieler's (*Pringsh. Jahrb.* xviii. p. 82) estimate of the pressure in the developing medullary ray cells of *Pinus sylvestris* at 21 atmospheres is the highest. It is clear that investigation of the osmotic capacity of leaves for high trees is wanted, also investigations of the variation in osmotic power produced by varying resistances in the flow of the current. The experiments of Pfeffer and others[3] show that the osmotic strength of cell-sap is capable of great adaptation to circumstances—cells respond by increased turgescence to various stimuli. Whether they can respond sufficiently to account for the ascent of water is another question.

My own opinion is that the question of resistance to the flow of water is a difficulty which the authors of the modern theory have not sufficiently met. Unless it can be shown that the resistance to the flow of water in wood is less than that indicated by existing researches, we must face the fact that we do not at present know of osmotic forces which we can suppose capable of raising water to a greater height than 40 metres.

Continuity of the water in the tracheals.—The theory we are considering apparently requires that there shall be continuous columns of water from leaf to root, because a break in the column means a collapse of the machinery. This seems at first sight a fair assumption, though I doubt its complete correctness. It is in any case worthy of discussion. It has been constantly insisted on by Sachs and others that at the time of most active transpiration the vessels contain air, and not water. It is therefore a violent disturbance of our current views to believe in continuous columns of water.

For evidence on this point we are chiefly indebted to Strasburger. It is a remarkable fact that he should, without any theory to encourage such a view, have come to the conclusion that approximate continuity of water columns is a condition of primary importance, and that he should have made out the cognate fact that the whole of the *alburnum* need not be simultaneously occupied by a transpiration current; parts of it may be so occupied, while parts of it are filled with air, and do not function as water-ways. This is a valuable contribution to knowledge, and to the adherents of the new theory it is priceless; the very existence of their hypothesis may depend on it.

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[1] Sachs' "Text Book," edit. iv., Eng. Tr., p. 679, describes evaporation taking place in the cell wall, which makes good the loss by imbibition.

[2] Strasburger (*Leitungsbahnen*, p. 777) observed equilibrium established a good deal quicker.

[1] Schwendener's experiments, *K. Preuss Akad.* 1886, p. 579, do not particularly bear on this question.

[2] It is possible that the rate of the ascending water is much less than is usually assumed. Thus Schwendener (*K. Preuss. Akad.* 1886, p. 584) calculates from an observation of v. Höhnel that the transpiration current in the stem of a tall beech was only 2 metres per day.

[3] Pfeffer, "Abhand. der k. Sächs," *Ges.* xx. p. 300; Eschenhagen, *Untersuchungen aus d. Bot. Inst. z. Tübingen*, 1889; Stange, *Bot. Zeit.*, 1892.

Strasburger's statements and reasoning are by no means accepted by every one; for instance, Schwendener refuses to take them seriously (*K. Preuss. Akad.*, 1892, p. 931).

Strasburger has microscopically examined the condition of the tracheals as regards air.[1] He found in the spruce fir in July "almost no air bubbles" in the wood of the current year, but air in considerable quantity in four-year-old wood. In the same month *Pinus Salzmanni* (*Laricio*) showed scattered bubbles in the spring wood of last year, and more in the autumn wood. In a larch there were only very occasional bubbles in the two last years' wood. In the silver fir the current year's wood was practically free from air: the air increased in the inner rings. *Tsuga canadensis* had no air in this year's wood, only a little in last year's, and an increasing quantity in the older rings, the fifth being very rich in air. In February, *Pinus strobus* had hardly any air in this year's wood, and the silver fir was all but free from it in the youngest ring. Robinia in July had the youngest wood almost air-free. *Ficus elastica* and *spuria*, various *Acacias*, and willows gave vessels not entirely free from air, but nearly so. He concludes (*loc. cit.* p. 688) that the path of the transpiration current is not absolutely free from air. The younger wood, which especially functions as the water-carrier, is the most free.

Dixon and Joly quote Strasburger's results, which they consider sufficiently favourable to their views. They rely, in addition, on the impermeability of wet cell-walls to air, isolating the conduits in which air has appeared; and on the possibility that the air may be redissolved under root-pressure (*Phil. Trans.* p. 572), an idea well worth testing.

I think Strasburger's facts are not so favourable to their theory as these authors believe; in the same way it seems to me that Askenasy is rash in saying[2] that the tracheals in many cases contain continuous columns of water. It is true that this statement does not affect the validity of his general argument, since he faces the undoubted occurrence of air bubbles in many cases. This is undoubtedly necessary, and, fortunately, we can once more turn to the *Leitungsbahnen*. Strasburger states that he has seen water creep past the air bubbles (*Leitungsbahnen*, pp. 704, 709; see also "Hist. Beitr." v. p. 76) in coniferous tracheids. The best evidence for this seems to be the fact mentioned (*ibid.* p. 79), that the part of a single tracheid in front of an air bubble gets red with absorbed eosin, though the neighbouring tracheids are colourless. This clearly suggests the creeping round the bubble which Strasburger believes in. Schwendener (*Zur. Kritik*, &c., p. 921) has been unable to confirm Strasburger's microscopic observations, and, moreover, denies the physical possibility of the phenomena. I am unable to judge of the validity of Schwendener's theoretic objections, and must leave this point. It is a question of great importance whether it is possible that, on the breaking of a column of water, a film of water remains surrounding the air bubble, and capable of holding the two columns together. If this is impossible, we must suspend our judgment until we know more of the contents of the tracheals.

To sum up this part of the subject, we may believe that the tracheals in their youngest condition may contain water in continuous columns, since the cambium cells from which they arise certainly contain fluid. But we know also that this condition is not absolutely maintained, since Strasburger has shown that the young wood contains air, though in small quantity. We must, therefore, believe either (1) that the transpiration current is able to travel past the air bubbles, or (2) that tracheals partly filled with air may again become continuous water-ways by solution of the air. If we adopt the first alternative, we must believe that the film of water between the bubble and the wall of the vessel is able to bear such a tensile stress that it can serve to link the column above with the column below the bubble. But this is analogous to trusting a rope so nearly cut through that only a few threads remain intact. With regard to the second alternative, we have, at least, indications from Strasburger's work that a tracheal partly filled with air does not necessarily remain permanently functionless (see *Leitungsbahnen*, p. 692).

The isolation of the tracheals.—There are a number of points connected with the structure and properties of wood which ought to be considered in relation to the modern theories. Want of space forbids my doing more than referring to two of them.

The resistance which the wetted cell-wall offers to the passage of undissolved air is a point on which many writers have laid stress. It is clear that on any theory of the movement of water in the tracheals, it is essential that air should not filter into the water-way. This necessity is not, however, stronger in the case of the modern theories we are considering. The pressure tending to fill the tracheals with air from outside cannot be greater than atmospheric pressure, and since the wetted cell-walls of gymnospermous wood can resist the passage of air under a pressure of about an atmosphere,[1] we need not fear criticism of the theory on this ground. The above remarks seem, however, to be needed in face of the frequently recurring statement that wet wood membranes are impermeable to free air. Schwendener has some good remarks on this head (*Zur Kritik*, p. 943).

Strasburger has called attention to the important subject of the localisation or isolation of vessels, or of certain lines of tracheids. When this is possible we may have one set of tracheals containing continuous water columns, while neighbouring ones contain air at negative pressure (see *Histolog. Beiträge*, v. p. 87). This is especially important in connection with the Dixon-Joly-Askenasy theory, since, if there were no such isolation, a functioning tracheal containing a continuous column of water would give up its water to one which was not functioning. In other words, the inactive tracheals would, by negative pressure, suck water from the active ones. In the coniferous trees, the young wood is cut off by the absence of pits in the tangential walls[2] from free communication with the older wood, where air is more frequent.

In the same way the valve-like closure of the pits by the aspiration of the pit membrane, comes to be a subject of much importance.

At present I merely wish to show by a couple of examples the necessity of a complete study of the minute structure of wood in relation to the modern theories. It is, at least, a hopeful fact for Messrs. Dixon, Joly, and Askenasy that we cannot point to anything in the anatomy of wood which is absolutely inconsistent with their views. Finally, with regard to the question at large, whether we are friends or opponents of Messrs. Dixon, Joly, and Askenasy's theory, the broad facts remain that water has the power of resisting tensile strength, and that this fact must henceforth be a factor in the problem. There are difficulties in the way of our author's theory, but it is especially deserving of notice that many of these difficulties are equally serious in the case of any theory which excludes the help of the living elements of the wood, and assumes a flow of water in the tracheals. The authors have not only suggested a *vera causa*, but have done so without multiplying difficulties. There is, therefore, a distinct balance in their favour.

Huxley, quoting from Goethe, makes use of the expression *thätige Skepsis*. It is a frame of mind highly appropriate to us in the present juncture, if we interpret it to mean a state of doubt whose fruit is activity, and if we translate activity by experiment.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

MRS. RANDALL, of Massachusetts, has bequeathed to Radcliff College (the Harvard annex for women) 20,000 dollars; an equal amount to the Prospect Union, and 70,000 dollars to the Foxcroft Club of Harvard.

THE Council of University College, Liverpool, on the recommendation of the Senate and Medical Faculty, have converted the Medical Lectureship of Hygiene into a Professorship of Public Health, and have appointed Dr. E. W. Hope, Medical Officer of Health to the Corporation of Liverpool, to the chair.

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[1] *Leitungsbahnen*, p. 683 *et. seq.*; Russow in 1882 (*Bot. Centr.*) vol. xiii. 1883) observed similar facts in the distribution of water and air.

[2] *Verhand. Naturhist. med. Vereins Heidelberg*, 1895, p. 15.

[1] *Leitungsbahnen*, p. 722. Nägeli and Schwendener, *Das Mikroskop*, 2nd edit. p. 367, give 225 cm. of mercury.

[2] Strasburger discusses, in this connection, the existence of tangential pits in the autumnal wood (see *Leitungsbahnen*, p. 713).